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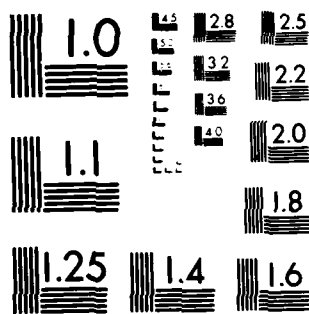
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of the Pre-Processing of Evidence

Lee Friedman, William C. Howell,

and Cary R. Jensen

Rice University

Technical Report #84-4

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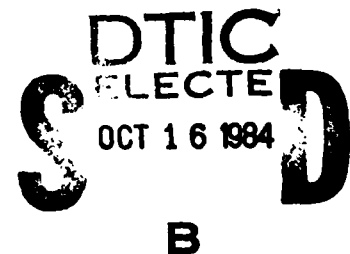
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Diagnostic Judgment as a Function of the Pre-Processing of Evidence

LEE FRIEDMAN, WILLIAM C. HOWELL, and CARY R. JENSEN, Rice
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Two experiments were conducted to determine how the quality of a human judgment (in this case, military threat diagnosis) is affected by various levels of pre-processing applied to the raw predictive events when such processing is carried out by the human and by a machine "aid." The subject was required to estimate the threat of attack on the friendly position (criterion) posed by levels of activity observed in various enemy positions (cues). These enemy positions differed in the degree of potential threat that they posed. Overall threat judgments were made under conditions in which a prior overt estimate of position activity levels was or was not required. Machine-aiding conditions were as follows: 1) no aiding, where the subject simply observed raw events in "real time" (Experiment 2), 2) automatic (Experiment 1 & 2) or self (Experiment 1) tabulation of events, and 3) automatic computation of events (Experiment 2). Finally, the rate of event occurrences was manipulated (Experiment 2). When subjects made overall criterion judgments (threat evaluation) intuitively on the basis of events observed in "real time", their performance improved markedly by interposing cue estimation, even if cue estimation was fairly inaccurate. If events were computed automatically, permitting a more "analytic" threat judgment, performance improved and the redundant estimation step was

not helpful. If events were merely tabulated, estimation was helpful, but to an extent midway between the raw-observation and automatic computation conditions.

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INTRODUCTION

A common task in military, medical, business and most other decision systems is that of diagnosing the aggregate meaning of a succession of equivocal predictive events -- test results, reports, indexes, observations. For example, the physician examines the patient's medical history, presenting symptoms, and test results in forming a medical opinion; the businessperson weighs economic indices, cost projections, and market analyses in judging the potential of a new product; the commander evaluates a stream of intelligence information in estimating the threat posed by an enemy force.

With the evolution of sophisticated technologies for obtaining and processing such predictive data, the demands on the human decision maker have grown, as have the possibilities for automating some or all of the component functions (Schrenk, 1969; Slovic, 1981). In fact, use of so-called "decision aids" -- particularly in diagnosis -- has become fairly common in contexts as varied as professional sports, medicine, business, and military C³I systems (Sage, 1981; Wohl, 1981).

Despite these advances, however, the question of how best to allocate decision functions between man and machine is still unresolved (Slovic, Fischhoff, & Lichtenstein, 1977). Part of the problem lies in our lack of understanding of exactly how human capabilities, task demands, and decision

quality are related. True, a mass of research has appeared over the last decade exposing various forms of human "nonoptimality" (Kahneman, Slovic, and Tversky, 1982; Tversky & Kahneman, 1974), but it remains to be seen how general these "biases" are and to what extent they degrade performance on actual decision problems (Cohen, 1979; Einhorn & Hogarth, 1981; Hogarth, 1980). Most of the research has dealt with a particular facet of judgment or choice in isolation, using whatever task seemed most appropriate for that particular function. Thus, for example, strings of numbers or other events have been used to assess frequency/probability estimation (Erlick, 1964); the classical urn-and-balls or bookbag-and-poker-chips problem has been a favorite Bayesian inference paradigm (Edwards, 1968; Peterson & Beach, 1967); general knowledge items have been used to study confidence in judgment (Slovic, Fischhoff, & Lichtenstein, 1976); numerical values attached to predictive "cues" have been preferred in policy-capturing and multiple-cue-probability-learning research (Hammond, McClelland, & Mumpower, 1980; Kerkar, 1983); and carefully structured lotteries have been the main vehicle for studying choice behavior (Payne, Laughhunn, & Crum, 1982; Tversky & Kahneman, 1981).

In their natural habitat, of course, decision problems are not conveniently structured into these elements. Rarely, for example, does a personnel officer choose job candidates merely by aggregating a set of "cue" or predictor

scores (as in policy capturing); more likely, he/she uses such "processed" data in conjunction with raw observations covering some of the same characteristics and others derived from interviews, reference checks, and work history. Thus it is hard to say how the well-established inferiority of man to model in developing and applying a cue-weighting strategy (Dawes & Corrigan, 1974; Goldberg, 1970) will affect the actual quality of candidate selection.

Similarly, a military commander may well be subject to biases associated with the heuristic estimation of event probability (Sage, 1981; Wohl, 1981); yet in practice, he/she may rarely make overt estimates, and the question of whether such biases will seriously affect his/her ultimate diagnosis or action cannot be directly answered. In a word, we have difficulty translating the available data on human cognitive limitations into decision system recommendations because we do not know (1) how paradigm-specific the limitations are, (2) how many of the basic cognitive processes actually occur in any particular decision problem or (3) how such processes, if they occur, act and interact to affect system output.

What we do know is that human judgment and decision making is subject to a variety of subtle, formally irrelevant task influences (Einhorn & Hogarth, 1981; Hammond, 1981; Howell & Burnett, 1978; Kahneman & Tversky, 1979). Further, it appears that merely requiring the decision maker to perform certain processing steps (such as

overt frequency estimation) on the way to a terminal response (such as diagnosis or action selection) can itself influence the quality of the output (Howell & Kerkar, 1982). In view of these considerations, it would seem useful to study the issue of function allocation in a more comprehensive fashion than has typically been done, using a task comprising more than a single facet of the decision process. The present studies represent a start in this direction.

The purpose of the two experiments reported below was to determine how the quality of a human judgment (in this case, military threat diagnosis) is affected by various levels of pre-processing applied to the raw predictive events when such processing is carried out by the human and by a machine "aid." In essence the paradigm extends the standard "policy-capturing" task to a situation in which the "cue values" (processed predictors) are derived from a more fundamental set of events (raw observations) by man, machine, or a combination. More specifically, the subject is required to estimate the threat of attack on his position (criterion) posed by levels of activity observed in various enemy positions (cues). The activity levels, however, are themselves a direct reflection of the rate of observed events over time. Thus with automated pre-processing of cues (activity levels) the task becomes a straight policy-capturing paradigm; with total manual processing, it becomes a typical "intuition" task; with manual pre-processing, it

becomes a structured, two-stage judgment task. Using this approach it was possible to examine directly the quality of the overall judgments as well as the various subprocesses involved in each functional allocation.

EXPERIMENTS

Two studies were carried out using essentially the same task and paradigm. Both involved (between-groups) comparison of overall threat judgments made under conditions in which an overt estimate of position activity levels was required (estimation groups) or was not required (no estimation groups) for identical sets of raw observations (cittings). Both studies also included a between-groups "aiding" manipulation. In Experiment 1 the aiding manipulation concerned whether event citings were tabulated automatically or whether subjects had to press particular keys to tabulate the events (automatic tabulation vs. self-tabulation). In Experiment 2 the aiding manipulation consisted of three conditions: 1) no aiding (subjects simply observed raw events in real time), 2) automatic tabulation, as above, and 3) automated computation of cue values. And finally, a within-groups manipulation (rate of citings) was incorporated into the second study.

In view of the similarities between the two studies, all common methodological features will be described here, and any unique features will be noted in the subsequent description of the individual experiments.

Common Method

Subjects. A total of 150 Rice University undergraduate students volunteered to participate in exchange for course credit or pay (\$4.00 per hour). The first 60 of these were assigned randomly to the groups comprising Experiment 1; the remaining 90 were assigned likewise to the six groups of Experiment 2. All groups in both studies, therefore, consisted of 15 subjects apiece.

Apparatus and procedure. Subjects served individually for a single session which lasted approximately one hour. During this time they completed 20 problems, each of which consisted of a series of citings obtained over a several minute period from four hypothetical enemy locations. Each problem terminated with the subjects' evaluation of overall threat posed for that problem. The entire experiment was programmed on a TRS-80 Model III microprocessor which was set up in a small experimental booth. Citings were displayed as flashing digits "1", "2", "3", or "4", each of which appeared in one of four respective areas of the CRT, the latter representing the four enemy positions. Depending upon the experimental conditions, citings were sometimes preserved on the CRT for the duration of a problem as small squares. These squares disappeared at the end of the problem when subjects were instructed to assess enemy readiness and/or threat. Responses, which were made via designated keys on the keyboard, were recorded

automatically.

Task. The instructions informed subjects that they were to serve as military intelligence officers responsible for monitoring activity in four regions controlled by enemy forces and for evaluating the overall threat posed to friendly forces. Enemy regions were designated according to their suitability as sites from which to launch an attack: Region 1 was the most suitable; Region 4, the least. Activity was defined in terms of citings yielded by combined surveillance systems: in Experiment 1, for example, 0-4 citings per region over the course of a problem was considered normal under peaceful conditions, 5-9 was moderate and could represent a build-up in readiness for attack, 10-14 was high and indicative of a significant build-up. Thus the subject was to consider both the activity observed (cue value) and the prior suitability of location (importance weight) in evaluating threat posed by any region; overall threat was the aggregate for all four regions.

In the course of a problem, the subject would see anywhere from 0 to 56 citings distributed across the regions. Distribution was varied over problems such that normal, moderate and high activity levels occurred in each region with equal frequency.

At the end of each problem, the subject was required to estimate overall threat posed on a scale of 1 (no threat) to

10 (attack imminent). In addition, he/she made an all-or-none "war-peace" judgment following everything else, or at any time during a problem when the perceived threat exceeded 5 on the overall scale. The "war-peace" feature was included primarily to encourage subjects to remain cognizant throughout the problem of their role as aggregator as well as monitor, and generally to help maintain interest. No explicit cost-payoff scheme was attached to it. In fact, instructions clearly emphasized that the numerical threat rating was the subjects' principal responsibility.¹

As noted earlier, one variable of interest was the presence or absence of a "cue-value" estimation requirement. In this task, activity level was the primary cue, hence frequency of citings (normal, moderate, or high) constituted the estimation requirement for those conditions where it applied. Therefore, estimation groups judged activity level for each region just prior to their overall threat evaluation, whereas no-estimation groups simply rated overall threat.

An objective threat index was computed for each problem by simply weighting each region's importance (1-4) by the number of programmed citings and summing over the four regions. Similarly, of course, an objective activity index was available in the

1 All-or-none data were analyzed but, since they yielded no information other than that reflected in the more precise ratings, they will not be discussed further.

number of actual citings at each location. Using these measures it was possible to calculate the accuracy of both kinds of judgments as well as the all-or-none "war-peace" response. In addition, by regressing threat evaluations (criterion values) on activity levels (cue values) it was possible to derive estimates of the subjective importance accorded each region (i.e. b-weights), and by comparing these weights to the assigned (1-4) values it was possible to evaluate the subject's weighting policies.

EXPERIMENT 1

Method

In this study, the principal questions were whether overt estimation of readiness at the four locations enhances aggregate threat evaluation, and whether automatic tabulation adds to that enhancement any more than self-tabulation does. The former manipulation was described previously. In the automatic tabulation condition, each event (represented by a digit) flashed on the CRT for .25 second, and was replaced by a small square that remained on the screen during the problem until subjects were instructed to assess readiness and/or threat. In the self-tabulation condition, subjects had to press a particular key ("1", "2", "3", or "4", depending upon the region where the event occurred) after each event in order to have it tabulated (preserved as a square). The design was a simple 2 X 2 factorial combination of these variables using four groups of 15 subjects each. The actual citing frequencies used in each region during a problem were drawn randomly from normal distributions over the

ranges 0-4 (mean = 2), 5-9 (mean = 7), and 10-14 (mean = 12) for "normal", "moderate", and "high" readiness respectively.

Results and Discussion

Correlations of threat ratings with objective values are shown in Table 1.

Insert Table 1 about here

A between-groups analysis of variance revealed a marginally significant estimation effect, $F(1, 56) = 3.58$, $p = 0.06$, but neither aiding nor its interaction with estimation approached significance, $F(1, 56) < 1.0$.

The findings using the more process-oriented "policy-capturing" measure, while consistent with the accuracy index, were a bit more clear-cut as shown in Table 2. The b-weights obtained under estimation conditions were considerably closer to the optimal values over the four regions than were those yielded by judgments made directly from observations (no-estimation conditions). In this case, a MANOVA was the appropriate statistical test, and the Hotelling-Lawley trace was used to approximate F . Here, the main effect of estimation was significant, $F(4, 53) = 2.76$, $p < 0.04$, and again, neither aiding nor its interaction with the estimation variables was significant, both $F(4, 53) < 1.30$, $p > 0.29$.

Insert Table 2 about here

In general, then, the results support the hypothesis that requiring an overt estimate of cue values enhances both the use of those cues in aggregate judgment and the overall quality of the threat assessment. The fact that automatic tabulation of events provided little additional benefit over self-tabulation may be attributable to a ceiling effect. Subjects in the estimation groups made correct frequency categorizations on 92% of the problems in both self and automatic tabulation conditions. Apparently, as long as events were preserved on the CRT, as they were in both aiding conditions, it did not matter whether the subject had to emit responses to preserve those events. It is worth noting, however, that even at this "ceiling" level, overt estimation enhanced the ultimate diagnosis. The second experiment included more demanding conditions.

EXPERIMENT 2

Our purpose of this study was to determine the replicability of the estimation effect found in Experiment 1 under a wider range of aiding and difficulty conditions. Another was to extend aiding to the point of actually calculating cue values (citing frequencies) as is typical in policy-capturing research. With these added conditions it was possible to compare threat evaluation (diagnosis) performance based on raw observations with that for partially and fully processed predictive data as discussed in the Introduction. The expectation was that aiding would help, but that the estimation requirement would serve much

the same purpose under conditions conducive to accurate readiness estimation. Under more difficult estimation conditions, of course, the relative effectiveness of the estimation requirement should decline since the overall threat assessment would be based on less accurate "cue values".

Method

The basic design replicated the automatic tabulation condition of Experiment 1 and added two levels of aiding -- 1) the direct computation of citing frequencies, and 2) an unaided condition in which subjects had to deal with observed events in real time. Thus it consisted of six groups obtained by crossing the estimation variable (two levels) with aiding conditions (unaided, tabulation, and computation). The difficulty variable was manipulated within subjects by using two levels of input rate: the easier condition, 3 min. per problem, was consistent with Experiment 1; the more difficult, 30 sec. per problem, was chosen to eliminate any possibility of actually counting the citings. The difficulty variable was applied only to the unaided and tabulation groups since the computation groups did not actually observe citings (so as to ensure that judgments were based exclusively on the processed cue values). Therefore, there were actually two designs: a 3 X 2 between-groups factorial with estimation difficulty collapsed, and a 2 X 2 X 2 mixed factorial with the computation conditions omitted.

The only other noteworthy differences in methodology between this study and the previous one were a slight increase in the

citing frequencies (a maximum of 20 rather than 14 in each region per problem) and a corresponding adjustment in the activity-level ranges (normal readiness was 0-6 citings per region with a mean of 3; moderate, 7-13 with a mean of 10; high, 14-20 with a mean of 17). Since the rate of citings was varied within subjects in the unaided and tabulation conditions, order effects were controlled by randomizing the presentation of slow and fast problems separately for each subject.

Results and Discussion

The data for overall quality of threat evaluations, again expressed in terms of mean correlations between obtained and optimal ratings, are summarized in Table 3.

Insert Table 3 about here

As predicted, performance improved systematically with level of aiding in the absence of any overt cue estimation requirement, but estimation alone produced substantial gains as well (from $r = 0.47$ to 0.79). In fact, the 0.79 compares favorably with the average for all aided conditions, which was 0.84 . The combination of aiding and estimation, however, added very little to either alone. Threat evaluation performance was not significantly different among the three groups who estimated readiness. Further, no significant differences in aiding (collapsed over tabulation and computation) appeared when threat evaluation was preceded by readiness (cue) estimation ($F < 1$). However, subjects in the tabulation/estimation group had

significantly higher correlations than those who had events tabulated but did not estimate readiness.

The above conclusions are supported by a highly significant estimation X aiding interaction, $F(2, 84) = 23.50$, $p < 0.0001$, and by post hoc comparisons of estimation with no-estimation means: $F(1, 84) = 66.36$, $p < 0.0001$ for the unaided condition; 4.82 , $p < 0.05$ for the tabulation condition; and 2.64 , $p > 0.10$ for the computation condition (reversed effect). The above conclusions are also supported by post hoc comparisons of means from the three aiding conditions: $F(2, 84) = 133.64$, $p < 0.0001$, for the no-estimation condition; $F(2, 84) = 2.73$, $p > 0.05$, for the estimation condition. Regarding the nonsignificant estimation effect for computation groups, it should be noted that the only readiness estimation involved in the computation condition was classifying the presented citing-frequency numbers into the proper readiness ranges. Despite the simplicity of this requirement, accuracy was not perfect (98%), which probably accounts for the nonsignificant decrement with estimation.

The difficulty variable apparently did not affect the quality of threat evaluations of unaided and tabulation groups. The difficulty effect was not statistically significant, $F(1, 56) = 2.23$, $p > 0.14$; nor were any interactions of difficulty with the between-groups variables. However, the estimation X aiding interaction was highly significant, thus substantiating the results of the between-groups analyses, $F(1, 56) = 26.69$, $p < 0.0001$. In Table 4 it appears that while estimating readiness

improved threat evaluations of both unaided and tabulation groups, it helped the unaided group considerably more.

Insert Table 4 about here

In contrast to the overall threat judgment, the accuracy of aided readiness estimates was unaffected by difficulty (95% for fast vs. 98% for slow conditions). However, the mean difference between unaided estimates for fast and slow conditions (76% vs. 93%, respectively) was significant, $t(14) = 8.53$, $p < 0.0001$. The fact that unaided subjects maintain accurate threat evaluations even when their readiness estimates are inaccurate constitutes rather definitive substantiation of the estimation effect. Even fairly inaccurate cue value estimates can lead to improved threat evaluations.

In sum, the results of these analyses indicate that when decision makers are forced to make overall criterion judgments (threat evaluation) intuitively on the basis of events observed in "real time", their performance can be improved markedly by interposing a processing step (cue estimation). However, if this processing is done automatically, permitting a more "analytic" approach to threat judgment, performance improves and the redundant estimation step is not helpful. If the event occurrences are merely preserved but not processed, estimation is again helpful, but to an extent midway between the raw-observation and the automatic processing conditions.

The above results are strengthened even further by the process (b-weight) measures as shown in Table 5.

Insert Table 5 about here

Separate MANOVAS for the three aiding groups yielded a significant estimation effect only in the unaided condition: the b-weights obtained with an estimation step were distributed more optimally than those obtained without one in this completely manual condition, $F(4, 25) = 2.93$, $p < 0.04$. While neither of the aided conditions yielded a significant estimation difference, $F < 1.0$, the trend under the tabulation condition was in the same direction as that for the unaided condition. It will be recalled that this trend also was apparent for estimation groups in Experiment 1. In particular both unaided and tabulation groups that are required to make intervening estimates of cue values tend to employ all of the cues in their overall threat judgments, whereas groups that do not estimate cue values tend to ignore all but the most predictive cue.

In the repeated-measures (i.e. $2 \times 2 \times 2$) MANOVA, the difficulty variable had a significant main effect on the distribution of b-weights, $F(4, 53) = 2.78$, $p < 0.04$, as did its interaction with the other two variables, $F(4, 53) = 2.68$, $p < 0.04$. Since the principal reason for this interaction appears to have been a poor distribution of weights under the unaided, no-estimation condition, the results are consistent with the conclusion that estimation helps most when conditions are

otherwise not very conducive to judgment. Surprisingly, this is true even though the estimated cue values under the unaided condition were 12% less accurate, on the average, than under any of the aided conditions.

CONCLUSION

The two studies reported here offer strong support for the proposition that higher-order, integrative judgments (threat diagnosis) benefit from the explicit "processing" of lower-order information whether carried out manually or through machine aiding. Conversely, and perhaps more importantly, serious deficiencies in the quality of diagnostic judgments are likely if the human decision maker draws inferences directly from a stream of "raw" observations. In such situations, he/she tends to limit consideration to the most predictive items, virtually ignoring lesser -- yet still very useful -- cues.

The tendency toward overselection in the use of diagnostic evidence has, of course, been reported before in other contexts (e. g., Nisbett & Ross, 1980). The typical explanation is that it represents a means of coping with information overload, a somewhat adaptive mechanism whereby the human compensates for his limited capacity by simplifying the environment (and perhaps losing some predictive power in the process). Neither information overload nor capacity limits, however, seem to account entirely for the present results. The estimation requirement added to, rather than subtracted from, the overall task demands, yet it produced a consistent improvement in unaided

performance even when the estimated values were not very accurate. Similarly, increasing the burden further by speeding up the input rate only enhanced the value of the estimation requirement (although, of course, it detracted somewhat from overall performance).

A more plausible explanation in the present case is that both the estimation requirement and machine aiding served to cast the predictive information into a form that was conducive to integration (increasing, in a sense, its compatability with the required cognitive operations). Such pre-processing presumably did simplify the ultimate integration step, but in a way that encouraged preserving rather than discarding predictive information. The important point is that without an explicit pre-processing step, subjects tended to simplify in other, less productive ways (overselection).

The results support Hammond's (1980) thesis that congruence between the decision maker's mode of cognition and the mode of processing induced by the task characteristics yields the most nearly optimal judgments. The nature of the threat evaluation task was such that it could be performed most optimally in an analytical framework. When cue estimation helped to provide that framework (by transforming real-time events into cue values), the decision maker's performance improved. The manual processing of cues may have shifted the decision maker from an intuitive to an analytical mode of processing. However, when an analytical framework was inherent in the task itself (through the automated

pre-processing of cues), cue estimation was not helpful. Finally, when the aiding condition provided a framework midway between raw events and pre-processed cue values, estimation was helpful to an extent midway between the raw-observation and automatic-processing conditions.

From a practical standpoint, the present results have two major implications. First, one cannot assume that the weighting strategies revealed through the typical policy-capturing study apply to "unprocessed" predictive data. Structuring the problem so as to provide the "judge" with explicit "cue values" dictates to an extent how he/she will integrate those cues.

Secondly, one does not have to incorporate machine aiding into the system in order to realize some of the benefits from structuring or pre-processing a stream of predictive evidence. The pre-processing can be done manually. This could be an important consideration in situations that, for one reason or another, preclude automated processing. The fact that merely requiring an estimation step can markedly enhance diagnostic judgment provides the system designer with a useful alternative. Of course, the present work is only a beginning; much remains to be learned about the influence of various forms of pre-processing on various kinds of subsequent judgments and decisions. We have examined but one set of processes in a fairly simple task setting. However, finding the pronounced effects that we did in even this limited context suggests that the approach is well worth pursuing into other, more complex, task domains. A

specific question in need of an answer is how far accuracy of manual pre-processing can decline before the advantage of that pre-processing is offset by the poor quality of the resulting cues.

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Table 1

Experiment 1

Mean Correlations Between Actual and Optimal Threat Assessments

Group	Automatic Tabulation		Aiding Self Tabulation	
	M	SD	M	SD
Estimation	.82	.09	.82	.15
No Estimation	.75	.21	.74	.12

Table 2

Experiment 1

Mean B-Weights of Each Region For the Estimation
and No Estimation Group

Group	1		2		3		4	
	M	SD	M	SD	M	SD	M	SD
Est.	5.41	1.31	3.14	1.24	1.48	1.60	0.82	1.07
No Est.	5.00	1.72	2.40	1.24	0.87	1.54	0.66	1.61
Optimal	4.00		3.00		2.00		1.00	

Table 3

Experiment 2

Mean Correlations Between Actual and Optimal Threat Assessments

Group	Aiding					
	None		Tabulation		Computation	
	M	SD	M	SD	M	SD
Est.	.79	.08	.86	.09	.83	.07
No Est.	.47	.18	.77	.10	.90	.07

Table 4

Experiment 2

Mean Correlations Between Actual/Optimal Threat Assessments

Aiding

Group	None				Tabulation			
	Fast Rate		Slow Rate		Fast Rate		Slow Rate	
	M	SD	M	SD	M	SD	M	SD
Est.	.77	.10	.82	.10	.83	.11	.90	.06
No Est.	.52	.19	.50	.25	.78	.12	.80	.11

Table 5

Experiment 2

Mean B-Weights For Each Region For the Different
Aiding and Estimation Groups

Unaided Group

	Region 1		Region 2		Region 3		Region 4	
Group	M	SD	M	SD	M	SD	M	SD
Est.	5.51	1.42	2.65	1.50	0.73	1.08	0.31	1.36
No Est.	4.12	2.03	1.52	1.80	-.07	2.09	-.88	1.74
Optimal	4.00		3.00		2.00		1.00	

Tabulation Group

	Region 1		Region 2		Region 3		Region 4	
Group	M	SD	M	SD	M	SD	M	SD
Est.	5.48	2.21	3.12	1.18	1.20	1.61	.38	1.20
No Est.	4.67	2.06	2.70	1.02	1.14	1.40	.05	2.39
Optimal	4.00		3.00		2.00		1.00	

Computation Group

	Region 1		Region 2		Region 3		Region 4	
Group	M	SD	M	SD	M	SD	M	SD
Est.	5.64	1.34	3.16	1.01	1.00	1.07	.78	1.39
No Est.	6.01	1.31	3.51	1.26	1.01	0.95	.33	0.88
Optimal	4.00		3.00		2.00		1.00	

LEE FRIEDMAN, WILLIAM C. HOWELL, AND CARY R. JENSEN

(Diagnostic Judgment as a Function of the Pre-Processing of Evidence)

LEE FRIEDMAN received his M.A. degree in social psychology from the University of Missouri in 1979. He is currently a graduate student in the Rice University Psychology Department, fulfilling requirements for a Ph.D. in industrial/organizational psychology. His research interests include judgment and decision processes, and personnel psychology issues. Until recently, Mr. Friedman developed and implemented a participative management program at the Texas Department of Human Resources, Houston, Texas.

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APPENDIX

Table 1

Experiment 2

Mean B-Weights For Each Region For the Different
Aiding, Estimation, and Difficulty (Fast Rate vs. Slow Rate)
Conditions

Unaided Group - Fast Rate

	Region 1		Region 2		Region 3		Region 4	
Group	M	SD	M	SD	M	SD	M	SD
Est.	4.96	1.58	2.12	2.08	0.84	1.34	0.66	2.05
No Est.	3.83	3.27	1.75	2.18	0.34	3.21	-1.94	2.52
Optimal	4.00		3.00		2.00		1.00	

Unaided Group - Slow Rate

	Region 1		Region 2		Region 3		Region 4	
Group	M	SD	M	SD	M	SD	M	SD
Est.	6.03	1.66	3.19	1.23	0.73	1.33	0.10	1.54
No Est.	4.13	2.20	1.28	1.97	-0.47	2.04	0.18	2.21
Optimal	4.00		3.00		2.00		1.00	

Tabulation Group - Fast Rate

	Region 1		Region 2		Region 3		Region 4	
Group	M	SD	M	SD	M	SD	M	SD
Est.	5.62	2.47	2.63	1.37	0.77	1.92	0.25	1.48
No Est.	4.94	1.93	2.18	1.08	0.33	1.68	0.01	2.72
Optimal	4.00		3.00		2.00		1.00	

Tabulation Group - Slow Rate

Group	Region 1		Region 2		Region 3		Region 4	
	M	SD	M	SD	M	SD	M	SD
Est.	4.95	2.48	3.62	1.27	1.62	1.74	0.51	1.17
No Est.	4.39	2.80	3.22	1.54	1.40	1.56	0.03	2.57
Optimal	4.00		3.00		2.00		1.00	

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